STANFORD UNIVERSITY MEDICAL CENTER
STANFORD, CALIFORNIA 94305 • (415) 321-1200

STANFORD UNIVERSITY SCHOOL OF MEDICINE
Department of Genetics

February 2, 1971

Dr. Glenn Seaborg
Chairman, AEC

Dear Glenn:

Thank you for your letter of January 30, and for the invitation to comment on your book. I will respond immediately to the marked passages and to your letter; then will study the full writing for possible further comment later. As you may know, I have been in intermittent contact with John Totter about this.

1. A large part of the public relations problem is the gap between the formally announced standards and the policy to which you (AEC) offer to adhere, and believe to be followed de facto. I can understand the conservatism with which you must act; we discussed this in some detail last year when I had the opportunity to meet with the Commission. Your present remarks tell me that there is even a larger margin than I had imagined; yet you are still inhibited from making full use of it.

I have then attempted to restate the issue in the form of the enclosed draft "Recommendations on Radiation Exposure Policy." I am sure that the promulgation of this statement would quiet the concerns of most of the scientifically informed public.

2. Trying to reconcile various numbers presented in the Knox report, I realize that a distinction may have to be made (more carefully than in my draft) between 'nuclear energy activities', and nuclear energy from reactors, and nuclear energy for civilian purposes. Rather than speculate about the role of non-civilian functions (which has residual fallout as an obvious component) I would suggest that you make the appropriate substitutions of wording in that statement to further public confidence in the civilian nuclear power program. If I accept Thompson's projection, there is a 30 db margin between current operations and the advocated 10 mrem policy, which should leave ample room for further expansion of nuclear power within the framework of existing technology. Since you evidently believe this, it is very difficult for the outside public to understand why you will not formalize it as a policy commitment.

3. Knox, p.17, refers to the possibility of a 'more refined calculation of the nation's annual dose distribution.' I strongly advocate that this be done.

SEABORG, Glenn

4. Your book: I can add very little to the marked passages.

The point made in the quotation @ p. 2-52 might be reinforced by attributing it to Neel and Schull's version, see attachment. (ABCC sounds very official and suspiciously like 'AEC').

As to your reference to me, p. 2-52a, I have little complaint also. To simply your discussion, let me substitute 'rem' for 'rad', which I should have done anyhow.. However, I should not say that the economic cost of present mutation only starts 100 or 200 years later; it will only be consummated over a lengthy period averaging some 5 to 10 generations. I would expect a significant burst of dominant effects to appear quite promptly; present data do not exclude that this penalty will be quite comparable ... to the cumulative one of delayed recessive changes. On the other hand most (90? 99?%) of the dominant effects will be embryo-lethals, inflicting minimum cost.

Anyhow, I would change the phrase '(starting about 100 to 200 years later)' to'(consummated during the next century, or later)' ; and similarly (that will occur 5 to 10 or more generations later), to (which will be spread out over a period of 5 to 10 generations).

The calculation of the actual temporal distribution of that impact is, needless to say quite difficult. Sewall Wright attempted it in a semi-qualitative way some time ago. When Cavalli-Sforza joins our department next week, I will try to interest him in joining this problem.

I agree with your concluding sentence, p. 2-52B, with the qualification nuclear radiation, within the range of present exposures,....

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N.A.S. 461 Neel + Schull 1956

classes 1 or 2) is approximately 100 *rep*'s. If we impose the restrictions indicated in the previous paragraphs with regard to an adequate test, then differences in the sex ratio between the control (average exposure 0 *rep*'s) and the exposed group (100 *rep*'s) as large as 1.6 per cent (absolute change) would quite probably not be detected. Accordingly, we may estimate that the yield, in man, might be as high as roughly 2 per cent per 100 *rep*'s and we would not detect it. This is a value six times that in Drosophila, but approximately one-half to one-third the value to be expected if human genes were as sensitive to irradiation as the small series of tested mouse genes (extrapolating from autosomal visibles to sex-linked lethals) and the X-chromosome of man had the same genetic length as that of Drosophila. This is, of course, the upper limit; the yield could be, and quite probably is, much lower. Similar conjectures could be made for a number of the other indicators.

Accordingly, we can say of the present study that *under circumstances where, on the basis of what is known concerning the radiation genetics of mammals, it appeared unlikely that conspicuous genetic effects of the atomic bombs could be demonstrated, such effects have in fact not been demonstrated.* The present study can in no way be interpreted to mean that there were no mutations induced in the survivors of the atomic blasts. Neither, on the other hand, is the reverse interpretation — that of mutation production — permissible from this series of observations, although, on the basis of all that is known of radiation genetics, there is no real reason to doubt that mutations were produced in Hiroshima and Nagasaki. We are left with inconclusive findings, albeit findings which permit us to set confidence limits.